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Dear Joshua,

Very exciting about your new job! I had heard about it only vaguely, and still don't really know quite what you're doing. I gather its teaching and research, which is much better than straight research as far as I'm concerned, if you have a light load and good students. I miss teaching a lot, and find that ideas don't crystallize well without a forum.

As you can see, I'm enclosing my last battered copy of my Symposium typescript. Forgive the hasty sketches which were the best I could do for figures, as I had no copies of those. The paper seems very outdated to me now, in the light of our new results. Nothing in it has been contradicted, but I would certainly write it very differently now! In case you might want to mention some of the recent work in the discussion, I can give you an idea of what has been happening. First of all, a number of controls have been added to the technique to take care of possible loopholes that occurred to us in the course of the experiments. One example: We worried about the possibility that some of the chemicals might cause delayed infection with phage (by coating the surface, or some such thing), thus permitting some division on the plate before lysis, and consequently production of spontaneous mutants. To check this, we now determine the rate of infection with phage of bacteria treated with the chemicals as compared with controls. Sofar, all are O.K. Desoxycholate-treated bugs seem to be more rapidly infected than controls, which was better than we hoped for. Our experiments to test for differential sensitivity to the chemicals of mutants and nonmutants is now much improved. Also, we are developing a few other mutations, completely independent as far as we know of the phage system, to use along with phage resistance. I was never happy about having just one class of mutants to work with -- too much possibility of being fooled by peculiar effects on the adsorption and lysis system. We now have several strains of B, each deficient for one known growth factor, which undergo reverse mutation to prototrophy (or do you prefer prototrophism?) at a very convenient spontaneous rate. All we need do to detect them is plate a large sample (10^9 or thereabouts) on minimal agar, and count colonies. They meet the Luria and Delbruck variance test beautifully, and are exceedingly easy to work with. We are just at the point of adding two or three of them to the routine battery of experiments, along with resistance to T_1. I'll be a lot happier about the whole thing if they work. Other things have been added to the technique, but it would take too long to go into them al Suffice it to say that it's a lot better than it was.

As for new results with chemicals, the early indications that practically everything works are being borne out, with the qualification that toxicity is a sine qua non of mutagenicity.

Latest positive tests have been obtained with colchicine, caffeine, formaldehyde, urethane (Bryson) and the most potent of all, believe it or not, is sodium chloride. We regularly get 10^4 mutants per 10^8 survivors with good old NaCl. All these things are positive only when bacteria are killed, and I have a few fancy theories about the relation of killing to mutagenic action.

There is one correction to be made in the script, and that is the statement that acriflavine is negative in Drosophila. Demerec has repeated acriflavine with an improved technique (much longer exposure) and has obtained clearly positive resuts. Sofar, no known descrepancies between bacteria and Drosophila, tho only a few of my compounds have been tried on the flies.

I am very sorry that I never answered your last letter, in which you raised a question about induction of radiation-resistance by ultraviolet. I don't know if you remember your point after all this time. In case you've forgotten, you wondered if selection during irradiation could have accounted for apparent induction of B/r. pS (B/r) at highest dosage shown on curve (1800 ergs) was 3.5 You asked if killing curve could break sharply enough between 1800 er and 3800 (dosage used in induction expt.) to give pS (d 3800, B/r) of 3.8, which it would have to be if selection is the answer. This information should have been included in the paper, but it is a legitimate heckle in view of the fact that it wasn't. Actually, pS B (d 3800 B/r) is about 6, which I think should answer the question. Sorry for the long, long delay.

I hope you will be able to return the typescript as soon as you're through with it, as I have no other copy, and the proofs have not yet come. Please write when you have time, and tell me how things are with you, and with sex.

Very best regards,

Evelyn

P.S. Summary & acknowledgements are not included in the paper - is this okay denovo?